May 14, 1949.

Dr. B.D.Davis,
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Dear Bernie:

I am glad to see the great progress you have made with the aromatic mutants.

Our interest in this matter is only incidental, so please make any use that you can of any information or suggestions that you may have gotten from us. It is very unlikely that we will publish our findings on SW-38, except possibly incidentally to a description of genetic results in S. typhimurium. So please feel free to say whatever you think is justified, and make only those credits that you think are necessary (none would probably be best8).

There has been so much trouble with reversion in SW-38, as you will doubtless find, that I can't be certain of the meaning of the different responses. I do think that phenol, cyclohexanol, catechol, hydroquine were active, and resorcinol and that resorcinol and α-naphthol were and benzoate /one/ were not. We concentrated on cyclohexanol and succeeded in convincing ourselves that it was active by using limited numbers of cells inoculated into agar with various supplements, but the results were still too vague for there to be much point in publishing them. The results prove nothing, and they point to cyclohexyl compounds hardly more forcefully than paper-chemistry does. On this tack, Snell suggested that a cyclic condensation of triacetic acid, comparable to the acyl condensations by which it is itself synthesized, would give the keto-form of phloroglucinol; we haven't tried this, however.

I appreciate your offer of the various stocks and chemicals, but I really don't think that we have any mutants or ideas that could add to the solution of the problem, and there is nothing that we could do with them that you couldn't do much better.

I think that I've already expressed my doubts as to the absolute reliability of biochemical mutants in <u>proving</u> biochemical pathways, and these apply particularly to the simple phenol responses. Further, even quite valid synthetic responses do not prove that the pathway we establish is the normal sequence; they merely show that the organism may be competent to go along it. I still think that the most important lead in aromatic synthesis is a note by Jeziersji & Frei, Acta Helv Physiol Pharm 4:395-400 (1946) on a bacterial c-hexanecarboxylic dehydrogenase. Have you seen this?

Sincerely,